Reply to the invited review by Niel Roberts

We thank the reviewer for his insightful and inspiring comments. We hope our answers and the changes we suggest in the manuscript are to his satisfaction.

- The validity of pollen-inferred climate changes during periods of major human landuse change (e.g. Late Holocene) has been a matter of debate. Consequently, it would be helpful if the results presented here could be compared graphically against other independent palaeo-climate data sets for the eastern Mediterranean. Although some of these data are cited, the only real comparison made is with the dendro-climatic resultsof Touchan et al 2007, which covers just the last millennium. An additional diagram might compare Gravgaz against records from sites such as Koca'in cave (Göktürk, Fleitman et al), Nar lake (Jones et al 2006, Woodbridge and Roberts, QSR 2011), Soreq cave (Orland et al. 2009 Quat Res), etc. Of course, palaeoclimatic records do not always agree, because a) dating and therefore correlation is not precise b) the records are not recording the same factors (e.g. seasonality) c) different regions experienced different climate histories. Comparison of different records allows these factors to be evaluated.

The important note here is that the dating and correlation between various records do not always agree, in part, because of the quality of the material/interpretation, or the available chronological framework. The most crucial factor is that different regions are subjected to different climate histories on a decadal to centennial scale. A good example on a larger spatial scale is the east-west climate see-saw, about which the reviewer has recently published (Roberts et al., 2012 - Global and Planetary Change). However, the location of a site is also crucial on a smaller spatial scale. This makes Sagalassos important, as Anatolia is subjected to climatic phenomena from the Northern Atlantic, Asia, and Africa alike, The existence of this problem has also been addressed in the recent review article of Luterbacher et al. (2012 –see also his short comment). Apart from recording various climatic periods and studying the interdependence of climate, vegetation, and human impact in that region, our data fill up a geographical blind spot (Western Taurus Mountains) in the knowledge concerning the precise timing *and* impact on vegetation and man during these climatic periods.
For the evaluation of pollen-inferred climate changes during this 2000 year (of is het 2500?) period of human land use changes, we pay considerable attention to the dendrochronological results of Touchan et al. (2007) Because this record is the only detailed climatic proxy which covers the same region as our core (be it only for the most recent millennium). It therefore is the most trustworthy record with which to validate our chronology and verify the sedimentological and vegetation responses to climate change which we observe at Sagalassos. Specifically the ways in which the local sediment dynamics at Gravgaz respond to regional climate change are fascinating, but this could not have been evaluated without the Touchan data. Nar Lake also receives considerable attention in the discussion chapter (section 5.2.2.), although admittedly mostly in order to comment on the differences in human impact between Sagalassos and Nar.
Upon rereading the manuscript, we must conclude that the abovementioned notions, concerning the impact of the location of a site on its specific climate and vegetation history, and our intentions to provide data concerning a region about which relatively little is known, are not sufficiently presented in our introduction chapter. The introduction will therefore be rewritten to more clearly present our intentions. Additionally, we agree with the suggestion of the reviewer to present a diagram comparing a number of important climate proxies. A diagram displaying the main dry and wet periods as recorded by a selection of important proxies, the nature of these proxies, and the nature and quality of their chronological framework, has been added to the introduction chapter. The resulting diagram both clearly shows how the various climate periods (such as the MCA and LIA) occur throughout the eastern Mediterranean, but that their start and end dates can vary considerably from place to place. Finally, although we do mention Nar lake, we mostly refer to the work of Eastwood (and Haldon for the historical framework). We shall add additional relevant references and expanded our comparison between Sagalassos and Nar. We also more explicitly mention at the start of our discussion chapter the special role of Touchan et al. in our paper.
However, we do opt to use the other proxies primarily to show that the bioclimatic periods we find indeed reflect well known climatic periods such as the LIA and MCA, and to demonstrate how the timing of these periods varies from location to location, indicating the relevancy of our own record.

- In the eastern Mediterranean, most records indicate generally dry and cold conditions during the Little Ice Age and warmer and wetter conditions during the Medieval Climate Anomaly (Luterbacher et al 2012; Roberts et al, Glob Planet Change 2012). The Gravgaz record is overall in good agreement with this pattern. It also shows good agreement with records such as the well-dated sequence from Nar lake back to ca. AD800, but less good agreement before that time. Although this could be due to different climate histories or different controlling factors, I note that the Gravgaz chronology shows some rather dramatic changes in sedimentation rate between 270 and 150 cm (AD300 to AD900). This hints at the possibility that either the 14C sample Beta-257421 may be in error, or that there was a hiatus in this record. In any case, it may be worth adding a sentence to comment on the possible age uncertainty in this part of the record.

A comparison with the oxygen isotope record of Jones et al. (2006) reveals that prior to c. 800 AD, Nar lake lags behind Sagalassos. The final wet period of the Roman Warm Period is recorded to start c. AD 250-300 at Saga, while it is clearly visible that a similar moist phase at Nar only starts after c. AD 500. Similarly, the Dark Ages Cold Period starts during the 7th century at Saga, while at Nar, it starts at c. 750 AD.

We do not think that these differences may have been caused by the presence of a hiatus, or other disturbance of the sedimentation, nor do we think it is likely that radiocarbon sample Beta-257421 is faulty. As we state in the Materials and Methods chapter (from page 3386, line 25 onwards), we have used the BCal program to implement Bayesian statistics in order to limit the chances of including erroneous carbon dates. This process did not eliminate Beta-257421. BCal is neither full proof, nor fool proof. However, this sample also did not coincide with any signs of a disturbed sedimentation, such as abnormally high pollen percentages, abnormally low pollen preservation, abrupt shifts in the sediment record (e.g. a drop in organic matter, or a peak in detritic matter). We see no reason to doubt the accuracy of this carbon date.

A noteworthy phenomenon in the age-depth relationship is a single interval (222-150 cm) where sedimentation rates must have been considerably higher, at least during part of that interval. Apart from this single event, the age-depth relation does not change drastically. We concluded that the high sedimentation rate, implied by samples Beta-263585 and Beta-257421, is caused by an event which caused the short term deposition during one or several events. We link the high sedimentation rate to the presence of two layers characterized by high non-carbonate detritic matter content (Calcium Carbonate being linked to evaporation, rather than sedimentation for Gravgaz). At page 3400 (lines 21-26) of the discussion section, this sedimentation event is linked to fire activity. The serious objections raised by this reviewer, as well as anonymous reviewer #1, concerning the validity of the CharAnalysis in its present setting will be discussed below.

In light of these criticisms, the abovementioned text needs to be adapted. The pollen record indicates that these layers were deposited under very dry conditions and at a time that the landscape was characterized by open vegetation. The text has been altered to reflect this.

The date for the start of the DACP as recorded in the Gravgaz record is therefore considered to be trustworthy. This date is not only based on the Gravgaz pollen record, but also on that of Bereket, which has its own separate chronological framework. Two separate records from two separate areas from the same region both indicate that the landscape became more open and drier at the same time. We have no clear answer for why there is this difference in timing between Nar and Saga, but the fact that Saga is subjected to an (Oro)Mediterranean climate (the timing of our climatic shifts indicating a large influence of the NAO), while Nar is subjected to a drastically different continental climate, may at least form part of the reason. It is of note that the altitudes of both Nar lake and the two sample sites at Sagalassos are relatively comparable.

- The authors comment on widespread evidence for climatic aridification around AD650, but in many records this occurs later, around AD750

We must note that we do not claim that dry conditions in the territory of Sagalassos start exactly at AD 650. We consider our chronological framework relatively sturdy, as illustrated by the relatively high number of carbon dates, the good match with the tree ring record of Touchan for the last millennium, and the fact that major climatic shifts occur simultaneously in both the Gravgaz as well as the Bereket records, despite both records possessing their own independent chronological frameworks. But regardless, our estimations are still based on AMS dates, and the assumption of constant sedimentation rates between AMS dates (unless there are clear signs of disturbance). It is because of

this that all ages mentioned in the manuscript have been rounded to the nearest decade. It is also because of this that we state in the titles in our results and discussion sections that the early medieval dry period (cf Dark Ages Cold Period) starts during the "mid 7th century AD". When we note that our mid 7th century date matches that of other records, we stay careful by saying on page 3405, lines 2-5: *"A climatic deterioration, occurring during the mid 7th century, has been recorded throughout the Middle East (Heim et al., 1997; von Rad et al., 1999; Lückge et al., 2001; Jiang et al., 2002; Lamy et al., 2006; Migowski 5 et al., 2006)".*
In our experience, we have seen several estimates for the start of a dry phase occurring sometime during the Early Medieval period. However, we were previously not aware of any trend towards the 8th century AD. In our experience, the 7th century or 1300 BP (and sometimes explicitly the mid 7th century, or even 650 AD), was a characteristically frequent occurrence in records from locations from literally all over the Middle East, even despite the above mentioned regional differences in the timing of most climatic periods. We provided several references in section 5.2.1. (also listed in the above quote), but the same general focus on the 7th century exists in other proxies o.a. from the Red and Black Sea (Lamy et al. 2006), lake Gölhisar (Eastwood et al., 2006 – Granted, that's also sw Turkey), the Aral Sea (Oberhänsli et al. 2007 – Showing a distinct dry peak during the mid 7th century, but rainfall never reaches values achieved immediately before AD 650, and continues to decline over the following centuries), the Dead Sea (Neumann et al. 2007), and the Israeli coast (Schilman et al. 2001 - This record shows how a strong moist trend recorded off the Israeli coast was abruptly reversed at c. 1300 BP).
In light of the remarks of the reviewer, and the extreme carefulness with which one should compare various proxies, we will alter the sentence quoted above to read "7th century", rather than "mid 7th century". We will also more explicitly mention the uncertainties involved in constructing a chronology based on a finite number of AMS carbon dates.

- Charcoal analysis. This study adds to the growing data set about Mediterranean fire histories. However, CHAR is best suited for records with contiguous, not interval sampling, since the latter can "miss" individual fire events. If I understand correctly, the Gravgaz and Bereket charcoals were recorded on pollen slides, taken at intervals of 6.7 and 13.5 cm respectively. It would be helpful if the authors could clarify this in the paper.

The reviewer is absolutely correct in stating that CHAR is best suited for records with contiguous sampling, the chances of missing a fire event growing larger as sampling resolution decreases. Our charcoal analysis doesn't cover a continuous record. But even without a contiguous analyses, we suggest that such analyses may still be useful. Even if individual fire events may be missed, a period characterized by many fires may still be detected (as what was done in Kaniewski et al. 2008). CHAR analysis may also help prove or disprove hypotheses concerning sedimentation events or rapid changes in vegetation by detecting and verifying the presence of fire events at selected crucial parts of the pollen/sediment record. The question here should rather be how discontinuous a record may be before the results become too untrustworthy. The average sampling interval for the Gravgaz record is of 6.7 cm or 46.6 year per cm. For the Bereket record, the average sampling interval is 13.5 cm or 56.9 year per cm.
Both the current reviewer, as well as anonymous reviewer #1 have stated objections, we therefore see no other option than to agree that our resolution is insufficient for constructing a reliable (approximation of the) fire history. But we do consider the abovementioned resolutions (in samples/year) sufficient enough to add to our knowledge of the fire history of the area, even if it does not fully reveal it. The CHAR analysis still clearly shows that no extended fire periods, as were recorded for Bereket during the Beysehir Occupation Phase, are present. Both the Bereket as well as the Gravgaz cores were also described in the field. No charcoal layers were visible in either core, supporting our hypothesis that fire activity was limited after the end of the Beysehir Occupation Phase. This notion is also supported by the results of the NJ analyses.
We have decided to keep the CHAR analysis as part of our paper, but will clearly state the limitations of this approach in our discussion and material and methods chapters, and rely more on the results of the NJ analysis when discussing the possible impact of fire. The previously unused visual description of both cores will now also be explicitly mentioned. Section 5.3. will be renamed and rewritten, as this section best focuses on post disturbance succession in general, rather than the impact of fire specifically.